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Dear Josh,

How are things with you and Esther? I have not heard of you for months.

As to paper, I have reluctantly got my draft out from where it has been"maturing" for many weeks, and am revising certain sections, mainly to clarify parts found experimentally to be obscure. I don't think any very large changes from the version I sent you, except that I now put.....as the level for discriminating between E and non-E cells, ; in discussion I shall pay a bit more attention to Bisset's 'budding' notion as a model to be, if possible, excluded for m c p. As to the nature of the latter, C.Q's researches on spontaneous production of motiles in O strains continue, and seem likely to establish one to one relationship of flagellum and m c p. Main evidence is from an O strain of S. para typhi C which has very low incidence of motiles belowc 25°, and, in some sub-lines, incidences up to c 90% at 37°. On changing back to 37° proportion of motile cells declines in way expected if no m c p initiated after change, and each motile cell gives rise, at room temp., to 1 to about 8 "uni-lines"; the important thing is that there is fairly good co-relation between distribution of numbers of flagella (Leifson's stain) and distribution of no. of "unilines" per motile 37°-grown cell. He is also on way to proving m c p is not the (extra-cellular) flagellum, since removal of all flagella by shaking a population of cells grown at 37° is followed by re-appearance of expected proportion of motiles at 20°. I have been doing a bit of work on flagella removal and synthesis myself: results so far a bit messy, but they look consistent with results predicted by assuming that m c p are never lost, secrete flagellum at an average rate (microns per hour) which is independent of their age, and that rate of fraying-away etc. is negligible. This leads to result that mean length of flagellum in exponential phase = c 1.4 x average rate of synthesis (microns per generation time per m c p (or flagellum). However, scatter in lengths is

wide so I think I miss many short ones which complicates calculations so I want to repeat with a Vibrio. The only relevance to abortive transduction is the evidence that of the delay from time of phage absorption to appearance of motile cells (translational; motility at first rotational etc. during re-growth) less than one generation time can be attributed to time required for synthesis of sufficient length of flagella, if they are secreted at same rate as in TM2.

Various other things in hand too, but none of much interest to bact.genetics (or anything else maybe). Madame de Margerie who before her marriage was called Hottinguer and worked with Ephrussi, is probably going to be working here with me (her husband is in French Embassy, and she wants to do some part-time lab. work). If all goes well I think we might take up again the serology of abortively transformed cells in SW 666, using cells from pedigree experiments. I think I told you my impression was that while nearly all initials are double-reacting, cells with unilinearly-transmitted motility come to react with anti-b only. Have you done any more on this angle ?

Helen B is now installed and working away at her F agent from line 3.

Could write more, but as I have been so long in writing at all I had better get this off first. Let me know the state of your 'abortive transduction 'draft: I think the side-by-side presentation is best idea. I saw the L & S abstract in Genetics; it seemed to cover the ground adequately, many thanks. I enclose a carbon of abstract of a paper C.Q. has just read to the Genetical Society here. Hope it is comprehensible.

Yours Emily Bruce

B.Stocker.

P.S.
(Helen asks if you received a culture she sent you, as requested)